

It may just be noticed (as a passing remark) that the total degree in the general case is the coefficient of

$$\rho^{\alpha} \cdot \sigma^{\beta} \cdot \tau^{\gamma} \dots \omega^{\lambda} \text{ in } P \left\{ \frac{1}{L} + \frac{1}{L_1} + \dots + \frac{1}{L_n} \right\},$$

and the *total* weight the coefficient of the same argument in

$$P \left\{ \frac{1}{\rho} + \frac{1}{\sigma} + \dots + \frac{1}{\omega} \right\}.$$

XIX. "Some Remarks appended to a Report on Mr. Hopkins's Paper 'On the Theory of the Motion of Glaciers'". By Sir JOHN F. W. HERSCHEL, Bart., F.R.S. (Referee). Received January 31, 1863.

A few remarks arising out of the perusal of this paper may perhaps not be considered as out of place on the present occasion. They are not meant as in any way impugning the author's views of the laws determining the fracture and disruption of glacier masses, or their application to glacier-phenomena in general, but in relation to the somewhat mysterious process of regelation itself, and to those generally recognized and most remarkable facts of the gradual conversion of snow into more or less transparent ice, and the reunion of blocks and fissured or broken fragments, under the joint influence of renewed pressure and of that process (whatever its nature), into continuous masses. If regelation be really a process of crystallization, it seems exceedingly difficult to imagine how the molecules forming the cementing layer between two juxtaposed surfaces can at once arrange themselves conformably to the accidentally differing axial arrangements of those of the two surfaces cemented. A maced crystal is indeed a crystallographical possibility; but then the axes of the two individuals cohering by the macle-plane have to each other a definite geometrical relation in space, as is well exemplified in the case of the interrupting film in Iceland spar. At the temperature at which "regelation" takes place (viz. the precise limit between the liquid and solid states), it seems to me very possible that the cohesive forces of the molecules of the cemented surfaces may be so nearly counteracted as to bring those surfaces into what may be so far regarded

as a viscous state as to permit (not indeed a sensible and finite change of figure of a small portion of the mass without fracture, but) a certain freedom of movement in the individual molecules, to some sensible depth within the surface, so as to allow of a *gradually progressive* deviation of *their axes* from exact parallelism, and thus to effect a transition from one crystalline arrangement to another—not by mauling, but by curvilinear distortion, such as may be conceived to prevail in pearl-spar and other similar disturbed forms of crystals. Nay, I can conceive it possible that by very long continuance at this exact temperature (especially if aided by tremors short of disruption propagated through the mass, which, as we see in the crystallization of cold wrought iron in the axle-trees of railway carriages, powerfully favour the crystalline rearrangement of molecules even in the most rigid solids) the contiguous blocks may influence each other's crystallization to a greater and greater depth through the medium of the cementing film, thus tending continually to straiten the curve of the connecting chain of axes, and after a very long time to bring the two blocks into perfect conformity, so as to form an uninterrupted crystal; and this, or something like this, I take to be the process by which the snow of a *névé* is converted into the imperfectly transparent and sometimes fully transparent ice of a glacier. Tremors of the kind here alluded to would not be wanting in a glacier in continued process of displacement, and in some part or other of which disruptions consequent on violent strain are momentarily taking place.

On the subject of the temperature of the interior of a glacier, I would observe that there will be found in the archives of the Royal Society, on the occasion of the Committee for recommending objects of inquiry to Liéut. Foster during one of his Polar expeditions, a recommendation of mine that the expedition should be furnished with a set of boring-implements for the purpose of piercing some very large and compact mass of ice, with the expectation of finding it much below the freezing-temperature. The heat of summer, it was suggested, would all be carried off in the water resulting from surface-melting; while the intense cold of a polar winter would penetrate the interior, and thus give rise to a *mean* temperature very far below that of the external climate. The implements (if I remember rightly) *were* furnished, but Liéut. Foster reported that no mass of ice sufficiently large could be met with so free from fissures as not to be

permeated by infiltrating water during the summer months; and if any results were obtained, they were not striking or definite enough to be worth recording. That the lower surface of a glacier in contact with the earth is in a constant state of fusion, even in those cold regions, is proved by the phenomena recorded by Dr. Kane of the Mary Minturn River in lat. $78^{\circ} 54'$, and the feeder of the Kane Lake, lat. $78^{\circ} 18'$, which never ceases to flow, summer or winter. Admitting this as a general fact, the sliding of a glacier on its bed is an obvious necessity; and that it should be unaccelerated is no more a matter of wonder or difficulty of conception than the unaccelerated descent of the weight of a clock which is never abandoned to its own impetus, but brought to rest after every momentary descent by the action of the scapement,—or the unaccelerated fall of a body in a resisting medium when the resistance becomes equal to gravity,—or of a weight gradually and uniformly lowered by the hand. Perhaps the more general way of conceiving it would be to regard the whole glacier as a mass propped up against a support anyhow inclined, and prevented from tumbling over sideways by lateral stays. Such a mass would rest in its position, if duly supported either by a base-abutment, or by a heap of its own *débris*; but if these were slowly abraded, destroyed, or picked away, the whole mass would descend bodily in the exact manner of the withdrawal of support.

On the disruption of a nearly homogeneous elastic solid in a state of strain, I would add a remark which seems to me of some moment, as explanatory of the greater cohesive strength which is well known to be imparted to cements, especially those of a resinous or gummy nature, by the admixture of extraneous matter in fine powder. If in such a solid there be one portion, however small, weaker than the rest, the strain being uniform, a crack will originate in that place. Now a crack, once produced, has a tendency to run—for this plain reason, that at its momentary limit, at the point on which it has just arrived, the divellent force on the molecules there situated is counteracted only by half the cohesive force which acted when there was no crack, viz., the cohesion of the uncracked portion alone. But if a crack anywhere produced be stopped from running by encountering a solid particle of greater cohesive force, fracture will no longer be determined by the accidental deficiency of cohesion at some weak point, but by the average resistance of the whole cementing mass.

It would, I think, be interesting to determine, on the more transparent portions of glacier ice, by the simple and easily applied test of polarized light, whether a definite crystalline structure prevail in its interior, and if so, in what direction the axis lies in relation to the lines of fissure in the crevasses. Nor is there any reason why the idea above thrown out respecting the mutual modification of structure of two masses cemented by regelation, at or near their plane of junction, should not be subjected to a similar test.

XX. "On the Absorption and Radiation of Heat by Gaseous and Liquid Matter."—Fourth Memoir. By JOHN TYN-DALL, F.R.S. Received June 18, 1863.

In his former researches on the absorption and radiation of heat by gaseous matter, the author compared different gases and vapours at a common thickness with each other. In the first part of the present communication he determines, in the case of several gases and vapours, the absorption effected by different thicknesses of the same gaseous body. His least thickness was 0·01 of an inch, and his greatest 49·4 inches; thus the thickness varied from 1 to nearly 5000. The apparatus employed for the smaller thicknesses consisted of a hollow cylinder, with its end closed by a plate of rock-salt; into this fitted a second hollow cylinder, with its end closed by a second plate of salt. One cylinder moved within the other as a piston, and by this means the plates of salt could be brought into flat contact with each other, or separated to any required distance. The distance between the plates was measured by means of a vernier. The cylinder was placed horizontal, being suitably connected with the front chamber used in the author's former researches, and the source of heat employed was a copper plate, against which a steady sheet of gas-flame was caused to play.

The absorptions of carbonic oxide, carbonic acid, nitrous oxide, and olefiant gas were determined by this apparatus, and such differences as might be anticipated from former experiments were found. Olefiant gas maintained its great supremacy over the others at all thicknesses. A layer of this gas, 0·01 of an inch thick, effected an absorption of about 1 per cent. of the total radiation. To show